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E. J. STONE, M.A., F.R.S., President, in the Chair.

H. J. Chaney, Warden of Standards, Board of Trade, 29 Chalcot Crescent, Regent's Park Road, N.W.;

B. J. Hopkins, 23 Weymouth Terrace, Hackney Road, E.;

Rev. T. Harley, 24 Amwell Street, E.C.;

were balloted for and duly elected Fellows of the Society.

*Note on some Criticisms made by Mr. Stone on the Methods available for Determining the Solar Parallax.* By David Gill, LL.D., Her Majesty's Astronomer at the Cape of Good Hope.

In the accounts which have appeared in the *Observatory* and the *Astronomical Register* of the interesting discussion which followed Professor Newcomb's remarks at the January meeting of the Society, there are some points raised which appear to deserve further notice.

I refer in particular to the criticisms of the President (Mr. Stone) on the Heliometric method of determining the Solar Parallax. If Mr. Stone is correctly reported, he gives the following criticism of the method:

"When the distance of a planet from two stars, one of greater and the other of less right ascension, is measured E. and W. of the meridian, by comparing the distance of each star from the planet as measured on the two sides of the meridian, we can get a determination of the parallax affected by no systematic errors but corrections for refraction, motion of planet in the interval, and small error due to error of scale over the change of distance measured. The result should, therefore, be an accurate one. But by taking each star we ought to get independent

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determinations; and if these agreed very closely, it would prove that any systematic errors must have been reduced to very small limits. When, however, we have made the results from the two stars agree by the supposition of a change of scale value, although the result deduced may be true, yet its truth is not apparent from the equations; the check is lost."

I may ask, in the first place, whether it is fair criticism to condemn a method because the critic supposes it will not bear a test which it was never contemplated to apply to it, which would give no additional weight in the elimination of systematic errors if it were applied, and which would involve a mode of discussion that is obsolete and unscientific.

Let it be supposed that I had applied to my observations of *Mars* the test proposed by Mr. Stone; that I had deduced from measures of the stars preceding and following the planet precisely the same parallax, what additional value would the final result have had? I should simply have proved that, on the whole, a correct value of the temperature coefficient of the Heliometer had been employed. The result would not have proved that the light of the planet was differently influenced by refraction from that of a star, nor would any error have been eliminated that is not eliminated in the method of reduction which I adopted. It is besides infinitely more scientific and exact to proceed by a method of reduction which involves no assumptions as to temperature coefficients, or absolute scale values, but which in itself contains all the data for the elimination of errors that may exist in the determination of these quantities, which, after all, cannot be termed *constants*, for reasons given in my Memoir on the *Mars* Observations (p. 56).

It is perhaps desirable to recall the mistakes of Wichmann in his discussion of the parallax of the Star 1830 Groombridge. These mistakes have been most ably pointed out by Döllén (*Bull. Phys. Math. Acad. St. Petersburg*, t. xiii.) to be due to the adoption of the absolute method advocated by Mr. Stone; and he also shows the truthful conclusions that may be drawn from the same observations by a method similar with that which I have followed. Wichmann himself afterwards frankly acknowledged his error.

It is, in fact, a principle that is now accepted by all refined and accurate observers, that in delicate researches the observations must be so arranged that the possible errors shall take opposite signs, and, as far as possible, eliminate each other.

The point in Mr. Stone's criticism which most surprises me is his statement that the truth of the result for the class of errors taken account of by the equations (for Mr. Stone only discusses these) is not apparent from the equations.

I took especial pains (pp. 116-125 of my *Mars* Memoir) to explain the formation of these equations, and the question of their rigid consideration of all the errors named by Mr. Stone is not a matter of opinion but of geometry. I would be greatly

indebted to Mr. Stone if he would point out where these equations are not geometrically true, at least far within all practical limits.\*

Mr. Stone goes on to say, "The method in one case is a very strong one indeed; but in the case of the necessity of supposing different scale values E. and W. of the Meridian, I must confess that I consider its strength problematical."

It seems to have escaped the notice of Mr. Stone that I have anticipated this objection, and at pp. 151-155 of my *Mars* Memoir I have given the results of a reduction in which the scale values E. and W. of the Meridian *are not supposed different but identical*.

The result of this reduction is in precise agreement with the more refined original reduction, and the close agreement of the separate results proves that the systematic differences of scale value E. and W. of the Meridian are confined within very narrow limits.

My contention, however, is that no matter how large the systematic errors of scale value produced by difference of temperature in the evening and morning observations may be, the final results will be entirely independent of such errors if reduced by the method employed in my original reduction. It is surely wiser and safer to assume that such errors may exist, and to employ a method of reduction by which they will be eliminated, than to adopt a crude and exploded method of discussion which ignores the existence of the carefully arranged means of

\* There is only one assumption in the formation of the equations which is not rigidly exact—it is, that the relative positions of the comparison stars are assumed known. The effect of this assumption is that the accuracy of the observed change of the planet's R.A. between the evening and morning observations depends to a small extent on the tabular difference of R.A. of the stars of comparison. (I confine myself, for sake of simplicity, to the consideration of one coordinate.) An error in this difference will affect the measured change of the planet's R.A. by the quantity

$$\text{Error of } \Delta\alpha \text{ of comparison stars} \times \frac{\text{observed motion}}{\Delta\alpha \text{ of comparison stars}}.$$

About opposition the greatest change of R.A. between the observations of the evening and following morning appears to be about 300'' of arc, and the comparison stars may be taken as 2° apart, that is, each 1° distant from the planet on opposite sides of it. If we suppose the probable error of the  $\Delta\alpha$  of the two comparison stars to be  $\pm 0''.25$  (which is a large estimate considering the number and excellence of the meridian observations), the effect of this error on the measured change in the planet's R.A. would be

$$\pm 0''.25 \times \frac{300}{7200} = \pm 0''.01,$$

which would produce an error of less than 0''.003 in the resulting solar parallax! When it is further considered that the final tabular motion of the planet is derived from the adopted star places, it is obvious that not the remotest systematic error can remain from the assumption in question.

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eliminating possible errors, and affords no measure of the real accuracy of the final result.

It is of course open for anyone to assert dogmatically that the only crucial check is to determine the parallax separately from the absolute distances measured from stars preceding and following the planet. But let us take a well-known and strictly parallel case to illustrate the fallacy of such an assertion. It is now pretty generally admitted that for field operations (that is, when star places are assumed known) the most accurate method of determining latitude is that known as Talcott's method. This consists in measuring the *difference* of zenith distance of two stars culminating at nearly equal and opposite zenith distances from the place of observation.

A refined observer who desired to determine his latitude with the greatest precision would also be careful to employ as many pairs of stars in which the Z.D. of the North Star exceeded that of the South Star, as pairs in which the Z.D. of the South Star exceeded that of the North Star.

The observer would then be in a position

1. To discuss the resulting latitude, supposing the screw value unknown, and to determine the screw value from the observations;

2. To trust to the general symmetry of the arrangement of the comparison stars for the elimination of any error in the adopted screw value;

3. To deduce the latitude from absolute zenith distances of the stars (which we shall suppose also to have been measured), and to form our estimate of the accuracy of the resulting latitude from the agreement of the absolute results from the North and South Stars separately.

The first of these methods of reduction corresponds with that employed in my original reduction of the *Mars* stars.

The next represents the second discussion to which I have referred, in which the scale value is supposed known, and not to change between the East and West observations.

The third represents the method advocated by Mr. Stone.

If Mr. Stone is consistent he is no less bound to maintain that the check is lost in Talcott's method because the latitude is deduced from the *differences* of the opposite zenith distances, and not from the *absolute* opposite zenith distances; and that the accuracy of the resulting latitude would be more fairly represented by the agreement of the latitudes deduced separately from the absolute altitudes observed N. and S. of the zenith, than from the difference of Z.D. of pairs of opposite stars. He would, in fact, ignore the powerful elimination of errors of flexure and refraction secured by Talcott's method, in the same way that he would ignore the elimination of error of scale value and refraction in the Heliumeter method which I have advocated.

If, in fact, Mr. Stone insists on the absolute method in the case of the Heliumeter, as opposed to the elegant method of dif-

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ference, I do not see how he can avoid insisting on it in case of the Talcott latitude method; and if he does so, it is safe to assert that his opinion will not be seconded by that of any competent living astronomer who has ever made a refined investigation with one instrument or the other.

Mr. Stone's criticism generally does not, however, touch the real weak point of my result obtained from the *Mars* observations. That point is the possibility, and even probability, that the average light of *Mars* is of different refrangibility from that of the average light of the comparison stars, and that on account of the ruddy light of *Mars* it is not impossible that my value of the solar parallax is slightly too great.

Possibly partly to this account, partly to chromatic dispersion, is due the large value of the solar parallax that has been obtained from meridian observations of *Mars*. We have no data by which to estimate how far it is possible there should exist, besides, a systematic error in measuring zenith distances, having the same sign for nearly all observers. There are not wanting instances of similar nearly universal errors—errors whose common sign may be attributed to the fact that all the observers are human beings more or less similarly constituted.

Thus in determining the personal equation of the Cape observers for stars of different declination, I found that there was a marked difference in the relative equations on opposite sides of the zenith. Accordingly, a number of zenith stars were observed in which each star was observed over the first two or three wires, with the observer's feet to the South, and over the last two or three wires with his feet to the North. In observing the next star the order was reversed, in order to eliminate errors of the wire intervals.

The following are the results so obtained; each result depends on the observations of a different night:—

	No. of Stars.	Mean.
	s	s
Finlay = +0.063 ± 0.017	21	+0.079
+ 0.079 ± 0.011	32	
+ 0.111 ± 0.025	16	
+ 0.078 ± 0.012	28	
Maclear = +0.145 ± 0.025	23	+0.122
+ 0.117 ± 0.017	23	
+ 0.112 ± 0.023	20	
+ 0.115 ± 0.019	24	
Pett = +0.056 ± 0.020	18	+0.015
+ 0.009 ± 0.019	16	
− 0.023 ± 0.021	22	
+ 0.016 ± 0.018	24	



	No. of Stars.	Mean.
Freeman = $+0.014 \pm .030$	24	+0.066
+ $.076 \pm .018$	11	
+ $.144 \pm .027$	23	
+ $.029 \pm .022$	17	

These results are taken out in the sense: clock slow, observer facing N.; clock slow, observer facing S.\*

Precisely similar results were afterwards obtained by the use of a reversing prism attached to the eyepiece, by turning which through  $90^\circ$  the star could be made to appear to move either from right to left or left to right.

Here, then, is a very marked error, having the same sign, common to all our four experienced observers.

Again, in the case of the meridian observations of the *Mars* stars, it was found that nearly all observers made the Right Ascension of the final stars too great.

Is it not possible, in face of these facts, that in observing zenith distances of a large and unsymmetrically-coloured disc like *Mars*, when viewed at a considerable zenith distance, there may be a very sensible systematic error, having the same sign for nearly all observers?

We have at least no proof to the contrary, and my friend Dr. Elkin has suggested a very admirable test. If *Mars* were observed at Northern and Southern observatories at an opposition when the planet is nearly as distant as possible from the Earth, and if such a systematic error as I have suggested really existed, then the parallax resulting from such observations should be still larger than that derived from the oppositions when *Mars* is near the Earth.

There are excellent opportunities for making such determinations in 1884 and 1886.

There is, however, one method of determining the solar parallax which I believe to be free from all systematic error. I refer to the method which I followed in the case of *Juno*. These observations were confessedly incomplete, and afford, in themselves, no crucial test of the method.

There is a test, however, which fortunately depends on no theoretical considerations, to which I believe no exception can be taken, and which is capable of being easily applied.

Anyone who has taken part in the recent measures of *Victoria* and *Sappho* will admit that it is impossible to distinguish between the appearance of either of these planets and that of a fixed star of similar brightness. It is, therefore, possible to test the method by employing a fixed star instead of a planet. We

\* These results may help to explain the systematic discordances which Dr. de Ball has found between the Right Ascensions of the Cape and Northern Catalogues.

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know that a fixed star has no sensible diurnal parallax, and therefore we may measure with the Heliometer its distance from two or more fixed stars, and determine whether there is any apparent change in the relative position of this star with respect to its neighbours, as the result of a series of measures made both East and West of the Meridian.

As an example of such a test, I give the results of my last measures of the kind.

The circumstances are far from the most favourable possible. The central star (*Sirius*) is not in the line between the comparison stars *a* and *b*, the angle at *Sirius* being  $165^\circ$ . Also East of the Meridian the stars are nearly horizontal, and West of the Meridian they are nearly vertical.

The circumstances of definition were not favourable, the images and steadiness being 2-3 (where 1 is the most favourable, and 3-4 the worst in which such measures can be attempted). The light of the bright star was reduced by wire gauze screens which make the image of *Sirius*, as seen in the Heliometer, quite similar in brightness and appearance to the 7th magnitude stars of comparison.

1882, December 24.

*Evening Observations.*

S. Time.	Sirius and <i>a</i>			Sirius and <i>b</i>		
	h	m	Refrac.	h	m	Refrac.
2 38.2	3683	"854	1.287	3685	"14	
3 15.6	84.497	1.184	.68	3 0.2	26.907	1.016
3 23.6	83.995	1.145	.14	3 32.7	27.075	1.016
3 57.2	84.240	1.094	.33	3 38.8	27.062	1.029

*Morning Observations.*

9 18.8	3683.069	1.763	3684.83	9 28.8	3626.200	1.673	3627.87
9 55.2	82.721	2.200	84.92	9 46.7	.071	1.853	27.92
10 3.2	83.172	2.354	85.53	10 13.4	25.556	2.213	27.77
10 32.4	82.155	3.088	85.24	10 22.1	25.724	2.380	28.10

These observations give in the mean

	Sirius and <i>a</i>			Sirius and <i>b</i>			<i>a-b</i>
	h	m		h	m		
Evening observations	3	18.7	3685"32	3	14.7	3628"05	57"27
Morning "	9	57.4	.13	9	57.8	27.93	57.20

Each separate result is the mean of two bisections, one in each position of the segments with respect to the zero point.

The four results for each star on each occasion form a com-

plete set in which every variation of the possible methods of bisection is represented, both as to the final direction of the motion by which the bisection is completed and with reversal of the Heliometer on its axis of rotation. These conditions are of course unfavourable to the apparent agreement of the separate results, but very favourable to the accuracy of the mean.

The very exact agreement in the value of  $a-b$  from the evening and morning observations is of course, to some extent, a matter of chance. From the discussion of a very large number of similar observations, which will soon be ready for the press, the probable error of the difference ( $a-b$ ) comes out  $\pm 0''.15$  for eight bisections of each star, made in the order and with the precautions above described, and the same observations agree in showing that the resulting measure of  $a-b$  is quite the same, whether measured East or West of the Meridian.

Now, if instead of *Sirius* we had a minor planet, and if a station were occupied near the Equator, and comparison stars employed of nearly the same declination with the planet, and on opposite sides of it, and the declination were small, this distance  $a-b$  would represent twice the parallax due to the altitude. That is, the difference between  $a-b$  in the evening and morning after allowing for the planet's motion would be *four* times the parallax at the adopted altitude of observation.

Put this altitude even so high as  $45^\circ$ , and we get for the probable error of the solar parallax from one night's observation

$$\pm 0''.15 \sqrt{2} \times \frac{\Delta \sqrt{2}}{4} = \pm 0''.15 \Delta,$$

where  $\Delta$  is the planet's distance from the Earth, that of the Earth from the Sun being unity.

If planets as favourable as *Victoria* and *Sappho* were observed, this would reduce the probable error of the determination of the solar parallax to

$$\pm 0''.12.$$

as the result of a single night's measures.

The method is not liable to the smallest uncertainty, if the instrument and observer have been first proved by the tests I have indicated. It is certain that from the observation of the oppositions of two or three minor planets by this plan an entirely reliable value of the solar parallax could be obtained, and that at a cost of one-fourth of the last British Transit of *Venus* rate, including the cost of a new Heliometer, which would afterwards be available for other researches.

I have added the latter part of these notes in answer to Mr. Stone's expression of his opinion "that none of our methods (for determining the solar parallax) can be considered as free from the



April 1883. *Mr. Tatlock,  $\lambda$  Ursæ Minoris.*

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suspicion of systematic errors." It would be of great interest if Mr. Stone would point out his objections to the method I have described and to the tests I have proposed.

*Royal Observatory,  
Cape of Good Hope:  
1883, March 6.*

*On the Position of  $\lambda$  Ursæ Minoris.* By John Tatlock, Jun.

(Communicated by Prof. T. H. Safford.)

The present paper is intended as a continuation of the paper published under the same title by Prof. T. H. Safford in the *Monthly Notices* for June 1878.

This paper takes up the subject where Prof. Safford left off, and has been prepared, under his direction, according to the method and formulæ given by him in Vol. IV., Part I, of the *Annals of the Observatory of Harvard College*, where he has given a similar discussion of this star, based on the data accessible in 1862.

The method given in the foregoing publication, and used in this discussion, is as follows: Let  $\eta$  be the correction to the assumed A.R. for 1855.0;  $\eta'$  the correction for  $n$  years to the assumed proper motion in A.R.;  $i$  the correction to the assumed Dec. for 1855.0; and  $i'$  the correction for  $n$  years to the assumed proper motion in Dec. In this discussion we have assumed  $n=30$ .

Also let  $\alpha'$  and  $\delta'$  be the star's A.R. and Dec. for the time  $t+1855$ , referred to the coordinate planes of 1855,  $\delta$  the star's Dec. at the time  $t$  referred to the planes of 1855,  $\xi$  the angle at the star, used in referring the proper motion for 1855 to the planes of  $t+1855$ , and  $\omega$  and  $\omega'$  the weight in A.R. and Dec. respectively. Now making

$$\frac{\eta}{15} = w, \quad \frac{\eta'}{15} = x, \quad i = y, \quad i' = z,$$

and also in A.R.,

$$a = \sqrt{\omega} 15 \cos \delta \cos \xi$$

$$b = a \frac{t-1855}{30}$$

$$c = \sqrt{\omega} \sin \xi$$

$$d = c \frac{t-1855}{30}$$

$$v = \sqrt{\omega} \Delta \alpha' \cos \delta',$$